

Interactive comment on “Phytoplankton distribution in the Western Arctic Ocean during a summer of exceptional ice retreat” by P. Coupel et al.

Anonymous Referee #1

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Review of

Phytoplankton distribution in the Western Arctic Ocean

By

P. Coupel et al.

There is so little knowledge about phytoplankton in the Arctic Ocean, in particular in the central part, that any publication of that sort would be greatly welcome! In addition, the publication is based upon an extensive data set and has a very strong physical oceanography base. No doubt: these are the publications that we need and wish to

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see published. And indeed, I am convinced that this manuscript will get published soon. But, it is there not quite yet. Major changes have to be carried out, as far as I can see.

The greatest draw back is the English. Now, as I do not have English as my mother tongue I am certainly aware of the challenge and I am off course in a similar situation when I publish. However, this rather long manuscript is full of mistakes and unusual (may be French?) wording. Some of the expressions are directly scientifically wrong. It takes a lot of time to read and to correct some of the phrasing. The authors make a disservice for themselves as they drain the enthusiasm of the referee. This has to be changed before one can talk about publishing. Examples. Unproductive phytoplankton communities (they may have a low productivity, but they are not unproductive). Stratification starts off. Nutrient availability drives the highest biomass.. heavy ice condition. . . .allowing light limitation. The poverty and the richnessof a shelf. The 2008 obtained phytoplankton abundance. . . . Basis for the marine trophic chain. In the high Arctic latitudes, mainly composed of deep basins. Yielding a high statio-temporal variability (creating the base for. . . .?). these ecological patters will be reorganising. The spring bloom of phytoplankton (the spring bloom is based upon phytoplankton. The warmed environments. Samplespresentd a heavy ice pack. . . .Last objective will be assessed. . . . Is based upon the method described by (why not “according to”?). and edged by a continental slope. Stations presented ice free conditions. Pigment agency of polar species. Collocated comparisons. PP/Chl a is called the productivity index or specific productivity, not the productivity ratio. Efficiency to draw down carbon and export it. These conditions are characteristic of new production together with efficient transfer to the upper trophic level and important carbon export (cite Parson et al here????). nutrient rich waters could be uplifted in surface. New type of water. At the opposite. . . .conducting to important sea ice formation. Ordinary associated. Why nutrients are still in enough concentrations. Has been already evidenced. At the opposite. Carbon biological pump. The ice decline year. Provides us evidences. In a close future. No enrichment of surface layers by sediments.

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I wish to oppose the use of the term Western (or Easter) Arctic Ocean. This is North American jargon that is utterly unprecise. It means west of the USA and Canada. However, the Chukchi Sea is to the east of Russia and do we expect that Russians also call it the Western Arctic? At latitudes above 70 degrees the ocean gets more and more curved and east and west do not explain much as long as one does not use latitudes. And the western latitudes start at the Greenwich meridian. May be the authors have been both at W and E latitudes during their expedition? I advise to go for geographic terms so that all know where the investigation took place. You could for example write "in the Pacific sector".

There is an extensive use of abbreviations and I am not always sure if the authors apply internationally applied ones or invent their own. In understand why abbreviations are used, but may be you could make a table that eases the reader through the text. Is the active melting zone (AZM) a colloquial term? Is it not the Marginal Ice Zone (MIZ) or the Seasonal Ice Zone (SIZ)?

Considerable emphasis is provided to CHEMTAX and that is fine. However, much less evidence is provided to calibrate against direct microscopic counts. What endeavours were made to quantify microscopically the smallest fraction? At time the manuscript reads like discussion of the CHEMTAX methodology.

2008 was not a summer of exceptional ice retreat. Since 2007 all summers had a major retreat of ice in the Canadian Basin. You should rather write "during the recent ice conditions".

4.1.1. is a part of the discussion, but what is carried out is a comparison of results.

On page 6937 the authors discuss the primary production in the SCM. They write: too low or too high irradiance strongly reduces the productivity. Such issues are presented in textbooks, not in a paper of this level. Further, there is a lot of literature about these aspects and it would be good to know if the authors know these papers? Any other investigation of phytoplankton in the Arctic Ocean over the last 20 years that

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could illuminate these issues? Along these lines is the application of the term specific primary production or productivity index. Could it be that the authors have not read enough of the dedicated literature?

How do Prymnesiophytes and Prasinophytes adapt to feed on regenerated matter, brought up by turbulent processes? How can the PWW sink deeper by density and carry their nutrient contents?

On page 6940 the authors introduce a 4th category of headlines. I think this is too much.

In 4.3 the authors make a significant mistake by mixing up climate variability with climate change. It is tempting, indeed, but if one has few measurements in a region subjected to climate change one should describe the variability before interpreting that the variability was so much bigger than "normal" that it must be climate change. Along these lines is the "exceptional" ice retreat in 2008.

Page 6947. Are there no publications anywhere else in the Arctic Ocean that suggest similar conclusions?

Essential work from the Beaufort Sea such as that of Tremblay is missing and that is a crucial lack. Where is a discussion of the extremely detailed work of Koji Shimadas group in the Canada Basin? What about work carried out by David Barbers group recently?

The figures were difficult to study due to their format and size. If they do not get significant bigger in the publication then they will be of no big use. They can hardly be evaluated by a referee because of their size. Figs. 8, 11 and 12 are a nightmare, indicating that the authors never thought about the reader.

To summarise, while I strongly support the publication of this extremely valuable data set I must say that the format, the style and may be even the focus are not appropriate for publication as yet. One gets overwhelmed by the wealth of data and looks for the

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specific focus that could guide one through the manuscript. Here is enough for two manuscripts that could be tightly linked to each other and published face to face. The authors should read more literature and consider that also some relevant research has done in the European sector or the adjacent Beaufort shelf of the Arctic Ocean.

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